3/5/51

My dear Lederberg,

I proved a bad enough correspondent in the past, so I hope you will not be surprised too much of the long delay of this reply. I have been abroad part of this time, in Britain and at a meeting of microbial genetics held in Copenagnen. Unfortunately I was the only person attending the meeting, who was working with bacterial recombination, most people participating being either biochemists or phage workers. An interesting account was given by Ephrussi-Taylor of her work on transforming principles in pneumococci, suggesting recombination; but, as far as her experiments go sofar, it might well be a mixture of different transforming principles. Pontecorvo gave a paper on the structure of the gene, relating to Roper's work on pseudoalleles in Aspergillus. I gave a paper on drug resistance, essentially centered on chloromycetin-resistance.

As this last subject, I have tried the experiment you suggested, of selecting for higher resistance from the cross of two independent first steps, and looking for recombinants among the progeny; but this and other experiments have not yet yielded decent evidence, of summation of resistance obtained through independent mutations. I am trying to repeat this with strains exactly marked, so that recombinants with given patterns of characters should certainly bear both resistance genes originated from either parent, and testing their resistance as accurately as possible. Differences are very often minute and require exact tests of resistance. One thing I have ascertained, in doing these fine tests of resistance, and that is that recombinants between a sensitive and a single step inherit either sensitivity or

or resistance, giving a clear cut bimodal distribution and excluding blending inheritance.

Unfortunately chloromycetin-resistance is unsuitable for the research of physiogenetics of drug-resistance, the mechanism of action of this drug being practically unknown. It is not due, anyhow, to a markedly higher destruction of the drug by resistant strains. Probably, sulphonamide resistance would be much more useful under this point of view. I am wondering whether bacteria might not provide the best available material for a research on physiogenetics of quantitative inheritance, especially for which seems to me the most important topic, there, that is the biochemistry of polygenes interaction.

I shall be grateful if you will keep me informed of salient observations in your laboratory; as to mine nothing really of interest has happened. Since I came back to Italy I had to waste much time outside the laboratory, or in reorganization of research, a waste which I hope will stop shortly.

Yours sincerely